ceived that when the absorbent sodium-vapour was very dense and the dark line very broad a peculiar bend outwards appeared in the spectrum at the ends, and on opposite sides, of the line. The cone of sodium vapour in the Bunsen flame acts as a prism with upward horizontal refracting edges. If glowing sodium vapour give dispersion, this cone should give, with horizontal rays passing through it, a vertical (though necessarily impure) spectrum; and if the rays have also passed through a glass prism with horizontal refracting angle, a spectrum of the form above described should be got. From the position, the refractive index of the vapour is greatest for those rays which are most deflected downwards. In agreement with the author's researches on solid bodies and liquids, the refractive index increases greatly as you approach the band from the red side, is less on the green side than on the other, and then quickly increases again. If an actual prism of glowing sodium vapour could be produced, one might observe, even with little thickness of vapour, indications of anomalous dispersion in the narrow absorption lines. Herr Kundt's attempts, however, to change the cone-shaped flame, by means of lateral plates of glass or mica, to a prismatic one, led to nothing.

In a recent paper on the theory of inconstant galvanic elements (*Wied. Ann.*, No. 6) Herr Exner contends that the so-called galvanic polarisation in elements has no existence. The distinction between a Daniell and a Smee element is merely quantitative, not qualitative. What does he mean?

GEOGRAPHICAL NOTES

We understand that a letter was received in London last Saturday from a member of one of the Belgian Expeditions in Central Africa, stating that he had met Mr. Thomson, with the African Exploration Fund's Expedition, on May 18, at a place some ten days' march from Simba's, so that the party had evidently found it necessary to return to the coast by the caravan route to Bagamoyo or Saadani instead of following the original plan of coming out at Kilwa. It is probable that the change of route was necessitated by civil wars among the native tribes. Mr. Thomson has thus had an opportunity, not contemplated at the outset, of passing through a considerable tract of unknown country between the south-east of Lake Tanganyika and Unyanyembe, and it is satisfactory to know that in so doing he has been able to visit Lake Hikwa and settle its proper position, which has been a puzzle to geographers for some time. In a map accompanying the account of Mr. H. B. Cotterill's journey with the late Capt. Elton northwards from the head of Lake Nyassa, this lake is placed with dotted lines in a position which is probably a good deal too much to the south and east of its true locality. The letter above referred to added that Mr. Thomson was in excellent health, and that he claimed to have traversed 2,000 miles of unknown country in the twelve months he had then been on the march. A telegram from H.M.'s Consul-General at Zanzibar, dated July 17, announces the safe return of Mr. Thomson and his party.

Mr. Alfred Rabaud, president of the Marseilles Geographical Society, has just published (Marseilles: Barlatier-Prissat) a *brochure* entitled "L'Abbé Debaise et sa Mission géographique et scientifique dans l'Afrique centrale," which is accompanied by a photograph of the deceased traveller.

Prof. R. J. Veth, president of the Dutch Geographical Society, has just issued in Italian (Turin: Guido Cora), "Notizia de Selajar et Isole Adiacenti," which is illustrated by an original map of Selajar and other islands of the Celebes group, together with a note by Signor Cora.

The *Travailleur*, with the French Government Expedition for the exploration of the Bay of Biscay, left Bayonne on July 17, having on board MM. Milne-Edwards, father and son, Vaillant, of the Natural History Museum; Fischer, assistant naturalist; Marion of Marseilles, Fohn, Perin, and the English naturalists, Dr. Gwyn Jeffreys and the Rev. Mr. Norman. The results of the expedition may be described at the Swansea meeting of the British Association.

NEW SCHEME FOR DIRECTING BALLOONS

M. Gabriel Yon, one of the directors of the great Giffard captive balloon, and Mr. E. A. Pearse of Bristol, have each published a pamphlet on the direction of aërostats. The balloon of each inventor is to be elongated according to the principles of the experiments tried by Giffard in 1852 and by Dupuy de Lome in 1871. The propeller is to be moved by a gas-engine in the Pearse balloon, and by a steam-engine in the Yon balloon. M. Yon proposes to use the gas of the balloon as fuel, but only in proportion to loss of weight produced either by uncondensed steam or by consumption of petroleum.

Nothing can be said to be really impracticable in the Pearse scheme, although Mr. Pearse lacks the aëronautical training which distinguishes M. Yon, an aëronaut who ascended with M. Giffard in 1854, and has witnessed all his experiments. The only essential difference between M. Giffard's scheme and the new system is the place given to the fan, which M. Giffard attaches to the car. Practice will only decide whether the alteration projected is an improvement or otherwise. The reason alleged for the change is the bringing of the fan nearer to the centre of resistance. But it obliges the aëronaut to give to his fans a very small diameter, which requires an immense number of rotations in a second, and consequently represents a loss of power.

The calculations appear to have been made with care by M. Yon and Mr. Pearse. A trial would be greatly desirable, although it is impossible to suppose that the aërial carriage of Mr. Pearse or the directing balloon of M. Yon can possibly bring aëronauts to the North Pole for their inaugural trip, they may be instrumental in eliciting useful facts. We may add to the peculiarities of M. Yon's scheme that he uses a small globe inclosed in the lower part of the aërostat called a compensation sphere, and connected by a pipe with a ventilator, for keeping intact the form of his aërial machine. Mr. Pearse does not appear to be convinced of the urgent necessity of abstaining in any aërial construction from every complication which can be avoided at any cost, and he suggests the adoption of some accessory organs which, although designed to help aëronauts, would tax too much the lifting power or the attention of the aërial sailor. Mr. Pearse supposes that he will be able to navigate the air with an excess of weight, and does not pay attention to the intensity of motive power required to counteract gravitation even in a partial manner. He should certainly take advantage of the pamphlet written by his French competitor, who deals mostly with facts belonging to the public, and on which nobody can, in the present state of science, raise any claim as being his own property.

Both these pamphlets are greatly in advance of similar productions, and are creditable to their writers. Mr. Pearse's pamphlet has been only published for private publication. M. Yon's is printed with a number of plates representing many details; but a directing balloon is so complex a matter that this part of the publication can hardly be said to be complete.

Having been the builder of M. Dupuy de Lome's balloon and one of his crew, M. Yon may be said to have witnessed all the great aëronautical constructions of the age. Next to M. Henry Giffard, of whom he claims to be the pupil, he is the most completely qualified aëronaut to work out the solution of the great problem to which a recent success in photography has given unexpectedly in some respects a practical result.

W. De Fonvielle

EXPERIMENTS WITH THE WIRE TELEPHONE, CHIEFLY ON STRONGLY MAGNETIC METALS

By a wire telephone is meant an instrument like that described in Nature, vol. xxii. p. 168. In most of the experiments mentioned below, the mercury cups there figured were dispensed with, as they are unnecessary when stout wires are used. A small ear-piece with a ferrotype plate was also used instead of the drum-head, whose special purpose was to reproduce music so as to be audible at a distance. For hearing close at hand the ferrotype plate is much superior; indeed with the drum many of the sounds alluded to below could not be heard.

So far as I can see yet, the most probable cause of the sound in the wire telephone, when fine wires of ordinary weakly magnetic metals are used, seems to be variation of the longitudinal tension arising from the variation of the heating effect of the current. It is of course quite possible that there may be a lengthening of the wire due to the passage of the current over and above that arising from the heat developed, although such an effect can scarcely be said to be certainly established by experiment as yet.

Besides this cause three others were traced in the course of my experiments: Electrostatic action, external magnetic action, and internal magnetic action.

The following experiment was made with a very fine palladium silver wire, about 13 cm. long, as sounder. I connected the violin and microphone with four Bunsen's cells in circuit with the primary of a small induction coil (resistance of primary ·27, resistance of secondary 44), while the wire telephone was put in circuit with the secondary. With this arrangement the music was reproduced quite audibly, although the quality of the notes was "wiry." This small coil had a movable core, consisting of a bundle of iron wires, and the sound was louder with than without the core.

I next tried a more powerful induction-coil (resistance of primary ·3, secondary 320), all the other arrangements being unaltered. The music could then be just heard, but no more.

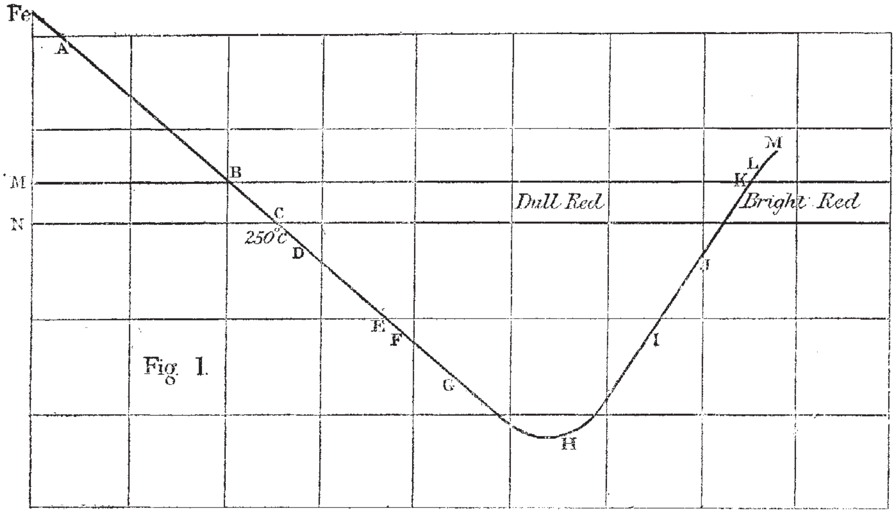

FIG. 1.—Iron.—A, sound very feeble at the temperature of the air; B, high note distinctly heard and increasing: C, feeble fizz now heard; D, fizz increased; E, quality of sound deepening; F, low note heard; G, sound very loud, low, medium, and high notes and buzz; H, no falling off; I, falling off now evident; J, marked diminution; K, fizz very soft, nearly gone; L, high note left; M, silence.

A large and very powerful induction-coil (resistance of secondary about 10,000), tried under similar circumstances, gave no result whatever.

Electrostatic Action.—As I have said, nothing was heard with the large induction coil when the secondary circuit was closed; but when it was interrupted at a mercury break, a loud hissing, rattling noise was heard. This could not have come by mechanical transmission from the induction-coil, which was several rooms off, the line wires being hung to the walls and jammed over three doors. It had its seat at the mercury pools of the break, and was doubtless due to electrostatic action. Similar sounds, only weaker, were observed with the smaller Ruhmkorf when the circuit was broken.

If two small disks separated by a small air-interval were made the terminals of an induction-coil, in the primary of which an interrupted current flows, they would form a condenser, and the difference of potential between them would vary in unison with the primary current. Consequently the electrostatic force of attraction would vary, and the disks, being set into vibration, would act as a telephone. The sounds in Thomson's singing condenser are probably due to this cause.

I have not attempted to carry this idea into practice, but I believe that telephones have been made on this principle by Edison and others.

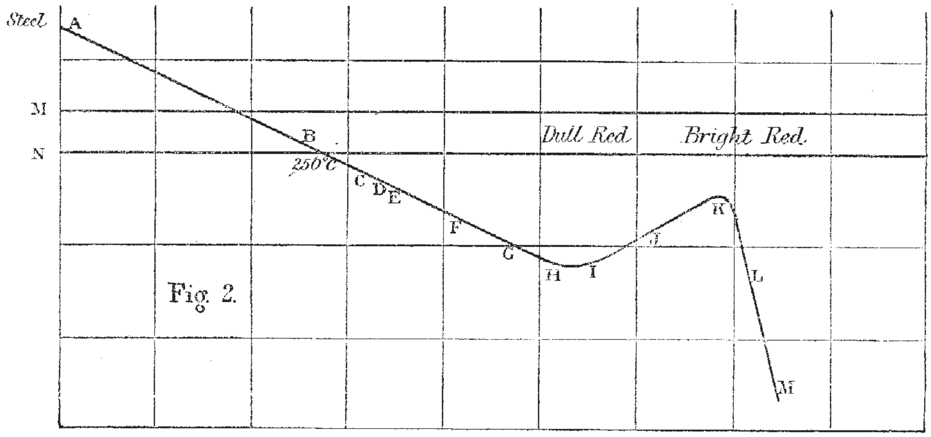

FIG. 2.—Steel.—A, fizzing sound and high note, neither loud; B, silence; C, high note comes in again; D, sharp fizz; E, buzzing sound and general increase of loudness. The other letters as with iron.

External Magnetic Action.—If the stretched wire of the telephone be brought into a magnetic field so as to cross the lines of force, and an interrupted current passed, loud sounds are heard. I used a tuning-fork interrupter with two Bunsen's cells. When a thick copper wire was put into the telephone, at first nothing whatever was heard; but when a horse-shoe magnet was brought up, and held with its plane perpendicular to the wire, the note of the fork was heard very loud (much louder than in the neighbourhood of the fork itself, in fact), and comparatively pure. Little or none of the hissing or buzzing sound of which I shall have to speak by and by can be got in this way. It makes no great difference to the sound produced in this way whether we use a wire of 2 mm. diameter or a wire ·3 mm. in diameter. With the thin wire, however, the visible amplitude of the transversal vibrations is much greater than with the thick. Using a brass wire 15 cm. long and ·4 mm. in diameter, I obtained with a tolerably powerful horse-shoe magnet transversal vibrations of 2 mm. amplitude or more.

The wire telephone, when used in this way, is pretty sensitive to magnetic influences. The presence of the pole of a bar magnet could be detected at a distance of several inches from the wire. It might be used to explore the magnetic field in a rough way. I found, for instance, that when I brought up a north pole on one side I could neutralise its effects by bringing up a north pole to a proper distance on the other side.

To get these sounds it is by no means necessary to have any elaborate arrangement of stretched wire and so forth. If a magnet be brought up to the wire leading to the telephone, the sound will be heard quite distinctly. If the wire be grasped tightly in the fingers between the magnet and the telephone wire, the sound is stopped, showing that it is transmitted mechanically along the wire. This experiment is certainly not new, but, although I have seen the possibility of such action mentioned (*e.g.* Wiedemann, "Galvanismus," Bd. ii. p. 602), I have nowhere seen any indication that the sounds are so marked and so easy to produce. I believe that this cause has been at work along with others in many experiments on the sounds obtained in magnetisation; for instance, in De la Rive's experiments. It is impossible, however, to decide with certainty, because no sufficient indications are usually given as to the nature of the magnetic field in which the wire conveying the interrupted current was placed.

The wire telephone arranged in this way with the wire in a strong magnetic field is well suited for reproducing music. Whether it could be adapted for articulate speech, I do not know.

The above experiments of course raise at once the question whether the sounds in the ordinary wire telephone and those I shall describe presently may not be due to the earth's magnetism. To settle this point, I stretched a brass wire 15 cm. long in the telephone; the wire was fine enough to give a feeble sound of itself when the interrupted current of two Bunsen's cells was passed through it. I shifted the apparatus about, so as to bring the wire as nearly as possible into the line of dip, and then placed it perpendicular to that position; but I could not detect the slightest change in the intensity of the sound. If it be borne in mind that here the distinction between wires as to their thickness is only important in so far as it affects their stiffness, it will, I think, be clear that this experiment settles that the earth's magnetism is not an operative cause with the current strengths I generally used. Another proof of this will be given by and by.

Effects due to the Magnetism of the Telephone Wire itself.—The following experiments were made with a view to test a conjecture of Prof. Tait's, referred to in a letter to NATURE, vol. xxii. p. 168, and to settle, if possible, the cause of the exceptional behaviour of iron wires in the experiments of De la Rive and Dr. Ferguson.

Two Bunsen's cells were used throughout, and the current was interrupted by a tuning-fork driven by an auxiliary battery.

My first experiment was made with an iron wire (A, 19 cm. long, ·50 mm. diameter). It gave a moderately loud sound to begin with, a low note with a predominating fizz, not unlike the fizz heard at the mercury cup in the far room (owing, I suppose, to the volatilisation of the spirit by the heat of the spark, which passes when the dipper of the tuning-fork leaves the mercury).

When a portion of the wire was heated with a Bunsen flame the sound increased very much for a short time, and then died away again considerably after the wire got red hot. On allowing the wire to cool, the sound, after a short time, suddenly swelled out and then fell away again. The permanent sound was, however, louder than it had been at first.[1]

I soon satisfied myself, by cautiously bringing the flame up to the wire, that there is a certain temperature at which the sound is a maximum. The wire was heated up to white heat and allowed to cool pretty rapidly, and it was found that the sound was at its loudest at a dull red heat, just before the phenomenon of the re-glow occurred, along with which a peculiar crackling could be heard, due, no doubt, to the abnormal contraction and extension of the iron at that temperature.

Several causes at once suggested themselves. The alteration of the elasticity of the wire was dismissed as probably not the *principal* cause at all events; for the increase of the sound begins at comparatively low temperatures. Although I did not expect to find any such thing, I looked for a maximum of resistance at a high temperature by placing the iron wire in one circuit of a differential galvanometer, balancing it with an equal resistance in the other, and then heating. I found, as is already known, that the resistance increases with great rapidity after dull red heat, but obtained no indication of a maximum.

The most probable explanation seemed to be the magnetic properties of the wire. It is well known that the magnetic susceptibility of iron (that is, loosely speaking, its power to become inductively magnetised under the influence of a given magnetic force) is at its maximum about dull red heat; that it declines very rapidly at higher temperatures, and is almost insensible at a bright red heat. The coercitive force of iron, that is, its power to retain magnetism permanently, unaided by external magnetic forces, disappears at a much lower temperature.

The sound in the above experiment depends, therefore, upon temperature in the same way as the magnetic susceptibility of the iron wire. This is strong proof that the sound is simply due to the fact that the iron is magnetised. I convinced myself by direct experiment that the effect extends throughout the whole of the wire, for I found that two flames at different places produced, when properly applied, more effect than one, and that, as I brought more and more of the wire to the proper temperature, the sound grew louder and louder. The fact that on cooling the permanent effect was greater than before probably corresponds to the fact that, under certain circumstances, the permanent magnetism is increased by heating and subsequent cooling, of which fact it was easy enough to make certain by testing the magnetism before and after heating.

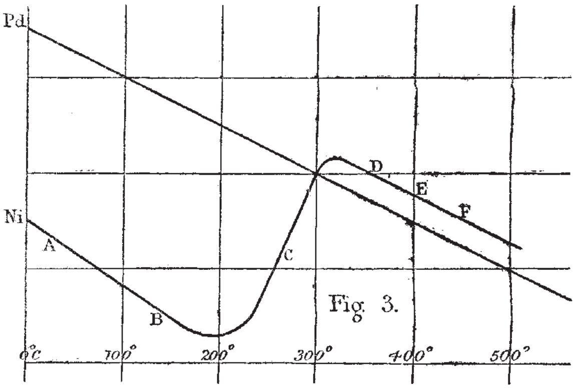

FIG. 3.—Nickel.—A, buzz and low note, both loud; B, low note, gone; C, buzz, tending to soften; D, buzz, gone; E, feeble high note; F, high note.

I next took a piece of steel piano wire (B, 20 cm. long, ·9 mm. diameter) and mounted it in the telephone. At first no sound whatever could be heard. On magnetising it longitudinally, by stroking once or twice with a pair of magnets, a sound was heard quite distinctly, viz., a gentle fizz accompanied by a high note. On magnetising more strongly this sound became somewhat louder, but retained the same character. Gentle heating with a spirit-lamp decreased the sound; but it recovered its intensity when the wire was allowed to cool, and remained permanently a little louder than at first. Repeated gentle heating and cooling increased the permanent sound somewhat.

The wire was then strongly heated with a Bunsen flame. At first the sound died away to a minimum, then it increased, and was very loud about a dull red, then it fell off again. When the wire cooled the sound rose to a maximum, and then fell off, no minimum being perceptible. After this the permanent sound was a good deal louder, but the diminution on *slightly* heating could no longer be observed with certainty.

I next heated the steel wire B to white heat throughout, so as to deprive it entirely of all magnetism, and tempered it by dropping it into cold water when dull red. When put into the telephone after this treatment it gave no sound whatever. One stroke with a pair of bar magnets caused it to sound quite distinctly. It gave a gentle fizzing sound along with a very high note. Repeated gentle heating and cooling gave the same results as I had got before. I then gradually raised the temperature till part of the wire was bright red, and finally allowed it to cool. As the temperature rose, the original fizzing sound died out, then the high note became inaudible, then there was a short interval of almost complete silence; after that a high note came in, then the fizzing sound again, which very quickly changed into a deep buzz, accompanied by a very low note like that of the tuning-fork, a note of medium pitch, and a high note (and possibly others), then the buzz died out, and at last the high note was left. When the wire cooled, the phenomena recurred in the corresponding order. First the buzz came back along with the low and medium notes, then it died away, and the high note alone was left; then there was silence, then the high note again, and lastly the fizzing sound.

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[1] This phenomenon was observed by Dr. Ferguson independently, and exhibited to the Royal Society of Edinburgh at the meeting before that at which an abstract of the present paper was read.

Most of the notes heard, certainly the most prominent of them, appear to have little relation to the tuning-fork. They seemed to be affected to some extent by the tension of the wire.

When a magnet was brought up to the wire the deep note, obtained in a similar way with wires of other metals, was heard along with those peculiar to iron and steel.

These experiments with the steel wire appear to me to settle the question as to the cause of the sound in thick iron wires. The fact that the wire can be put into a condition in which no sound is produced, and then made to sound by magnetising it, shows that the action is due to the magnetism of the wire, and is also an additional proof that the earth's magnetism had nothing to do with it.

This view is still further confirmed by the effect of heat on the tempered steel wire. The first effect of heat is to destroy the permanent magnetism of the wire, hence the initial diminution of the sound. At a temperature of about 250° C. the permanent magnetism is much reduced.

On heating farther, the magnetic susceptibility of the steel begins to increase rapidly, until it reaches a maximum about dull red, and then it falls off again very rapidly; hence the increase of the sound to a maximum and the final falling off.

The reason why the minimum cannot be observed with iron, and not always with soft steel, is that with them the permanent magnetism is less and the magnetic susceptibility in general greater at ordinary temperatures, so that the increase of the latter begins sooner and masks the decrease of the former. At a dull red all kinds of iron or steel are much on a par as to susceptibility; hence in the case of hard steel, whose susceptibility begins to increase rapidly only at a pretty high temperature, the phenomena are much more striking, as well as more varied than in the case of soft iron.

On cooling the sound came and went again as usual, leaving, however, a permanent sound of considerable loudness, which was increased by repeated operations of this kind.

As a test of the soundness of the above conclusions I was anxious to examine the behaviour of the other strongly magnetic metals, and Prof. Tait kindly put several pieces of nickel and cobalt at my disposal.

The piece of nickel used was 3 cm. long, 2 mm. broad, and about ·6 mm. thick. It was hard soldered to platinum terminals, and mounted in the usual way, after being heated red hot and dipped in water at dull red.

At first it gave a very feeble high note, accompanied by a gentle fizzing sound. One stroke with a magnet caused it to emit a loud buzzing sound. On heating gently this sound was somewhat reduced, and on heating farther the hissing sound died away, and a high note was left, but it too was extinguished before the nickel was visibly hot.

I made some temperature determinations by means of an air-bath and a mercury-thermometer, and found that at 200° C. the buzzing noise first began to be softened down. After 250° C. the diminution appeared rather more rapid, but at 350° C. the sound was still quite loud; after that the falling off was very rapid, and somewhere (say 400° C.) beyond the range of the thermometer, the mercury in which just boiled at the end of the experiment, the sound died out rather suddenly.

The behaviour of nickel is therefore exactly what we should expect from its magnetic properties, for it loses its magnetic susceptibility, according to Faraday and others, somewhere between 350° C. and 400° C.

I found with nickel, as with iron, that the current itself at a certain high temperature could produce much the same effect as I got by magnetising. On testing a piece of nickel after being magnetised by the current I found it to be transversely magnetised. This induced me to try magnetising my nickel strip transversely, but although I got results this way they were not so good as I had got by magnetising longitudinally.

I was thus led to try the following experiment, the result of which is at least curious. Instead of passing the current through the nickel itself as before, I passed it round two flat pieces of iron electro-magnet-wise. These were placed with their ends pretty close together, and the nickel was stretched between them so that it lay in a nearly uniform field of magnetic force, whose strength varied in unison with the interrupter.

I found that with this arrangement the nickel sounded very much as it did when the current was passed directly through it. The sound was not so loud, but its quality appeared to be the same. The sound, however, was loudest when the plane of the nickel strip was parallel to the lines of force, being very feeble when the plane of the strip was perpendicular to the lines of force.

A piece of watch-spring was tried in the same way, with exactly similar results.

This experiment is of course very nearly the same as some of those by which the sounds due to the magnetisation and demagnetisation of iron are usually demonstrated. A very full account of these sounds will be found in Wiedemann's "Galvanismus," Bd. ii. p. 565 *et seqq.*

I tried a piece of cobalt 6 cm. long and 6 mm. broad, ·7 mm. thick, in the ordinary way. In its original state it gave no sound whatever. After being magnetised longitudinally by a large number of strokes it gave a sound, very feeble, however, compared with that got in the same way from iron and nickel, or even from hard steel; it was, moreover, more of a pure note and less of a hissing noise. Heating in the first place diminished this initial sound, so that there came an interval of comparative silence, then the sound rose again, and by and by the familiar buzz came in; but up to a bright red heat no maximum was reached. On cooling, the phenomena reappeared in the proper order.

Cobalt behaves, therefore, just as we should expect from its refractory magnetic nature.

I may mention one curious phenomenon that appeared once or twice with cobalt and once or twice with a piece of steel. On cooling, after the maximum was past, the buzz had died away, and a period of comparative silence had come, strong beats began to be heard, which lasted for a considerable time, and then died away as the temperature fell. Various causes for these might be assigned. It might have happened that two parts of the metal were at different temperatures, and gave notes nearly in unison. It may very well have been interference between notes due to permanent and temporary magnetism; for in cobalt generally, and with the particular piece of steel in question, the minimum was not marked by the absolute silence which probably indicates cessation of the sound due to permanent magnetism before that due to temporary magnetism begins.

Relation to Thermo-electric Properties.—As it seemed to be of some interest to connect these magnetic sounds with the curious thermo-electric peculiarities of iron and nickel brought to light by the recent researches of Prof. Tait,[1] I asked the help of his assistant, Dr. C. G. Knott, who has had great experience in work of this kind.

The sounding-wire, a short piece of which was always used in order to get the phenomenon pure, was inclosed along with a double or triple thermo-electric junction in a small tube made by rolling up a piece of sheet-copper. The tube was then heated up in the blowpipe flame. This was a rough way of setting to work, but it was sufficient for our purpose.

The diagrams (Figs. 1, 2, 3), made by Mr. Knott, with the appended notes, will show the results. I have given the observations made during heating, as being on the whole probably nearest the mark. The cooling, except in the case of nickel, which was inclosed in a wide iron box, and did not require to be raised to a very high temperature, was much more rapid than the heating, and consequently inequalities of temperature due to the different positions of the sounding-wire and the junction would have been more apparent. In point of fact the discrepancy was not great.

The abscissa in the case of nickel is the temperature in centigrade degrees, in the other cases it is the electromotive force of a junction formed of a certain pair of platinum iridium alloys (called M and N) much used by Tait in his thermoelectric researches, because their lines on his thermoelectric diagram are nearly parallel (see p. 140 of the paper above referred to). The ordinate in all these cases is the thermoelectric power.

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[1] *Trans.* R. S. E., 1872, 3, vol. xxvii. p. 134, &c.

The special feature here is the period of silence at the neutral point of N and steel, viz., about 250° C.

This observation agrees remarkably well with the theory that the initial sound in the case of steel is due to its permanent magnetism; for, according to Faraday, steel loses its coercitive force about the temperature of boiling almond oil. See also Marshall (*Proc. R.S.E.*, 1871-72, p. 605). On cooling, owing doubtless to the fact that exposure to a high temperature had softened the steel, which was very hard to begin with, no period of absolute silence appeared, and beats were heard.

It was difficult to distinguish whether the note at E and F was or was not due to the singing of the Bunsen flame. The observations, on cooling, exactly corroborated those taken during heating.

It appears to me that these experiments establish that a series of sounds are produced by the passage of a varying current through magnetised iron, nickel, and cobalt, which depend on the fact of their magnetism. They are apparently of the same nature as those observed heretofore in magnetising and demagnetising iron.

I believe that the phenomena above described explain the exceptional power of iron wires of considerable thickness, as sounders in the wire telephone. When the iron wire is very thin it is most likely that the effect obtained with thin wires of ordinary metals predominates, and it is possible that the magnetic effect may in that case be very small. I cannot say, however, that I have settled this point, which clearly involves an experimental difficulty.

At all events I hope the above observations will be of sufficient interest to attract notice to a subject which has not been much studied lately, notwithstanding its important bearings on the theory of the telephone, and what is of more scientific importance still, the theory of magnetism in strongly-magnetic bodies, a department of physics which stands in as much need of additional light as any that I know. G. CHRYSTAL

INTERNATIONAL METEOROLOGY

THE International Meteorological Committee appointed by the Congress of Rome (1879), will hold its first meeting at Berne on the 9th proximo.

The following is the programme of questions to be considered by the Committee:—

1. Report on the action of the Committee since the date of the Congress at Rome.
2. Report of the Polar Conference (Weyprecht's project) held at Hamburg in October, 1879.
3. Proposed Conference for Agricultural Meteorology, summoned for September 6 at Vienna.
4. Proposed comparison of the Standard Instruments of the chief Observatories of Europe.
5. Proposed Catalogue of Meteorological Observations and of Meteorological Works and Memoirs in all languages.
6. Proposed International Tables for the reduction of observations.
7. Proposal for an International Meteorological Dictionary.
8. Report on the Meteorological Organisation of England in 1877, being a Supplement to the Fifth Appendix to the Report of the Roman Congress.
9. Proposal by Capt. Hoffmeyer for an International Telegraphic Service for the North Atlantic.
10. Proposal respecting the exchange of Meteorological Publications by post.

The Circular concerning the meeting, which is issued by Prof. Wild and Mr. Scott, requests all persons wishing to make any communications to the Committee to address them to Mr. Scott, at 116, Victoria Street, during the current month.

A private Conference on the relations of Meteorology to Agriculture and Forestry will be held at Vienna on September 6. The following is the programme of subjects for discussion:—

1. What are the mutual relations of the meteorological elements on vegetation, not only those which are proved to exist, but those which are theoretically supposed to be probable?
2. What observations of meteorological elements are to be particularly attended to, with especial reference to their influence on vegetation?
3. How far, and in what way, can meteorological observatories and stations, without interfering with their other work, include these observations in their sphere of operation?
4. Would it not be useful with a view of establishing special systems of observations for this object, as, for instance, phenological observations, to prepare general instructions?
5. Can, at the present moment meteorological central offices issue weather forecasts for the use of agriculture, with reasonable prospects of utility, and if this question is answered in the affirmative, how can the service be organised as fully as possible?

Preliminary materials for the answers to these questions will be found in the Reports of Dr. Lorenz and Dr. Bruhns to the Roman Congress on Article 35 of the Programme. These Reports have also been published separately in German, and partially in French in the collection issued by the Central Office at Rome of all Reports presented to the Congress. In the latter volume the Report of M. Denza on the same subject is to be found.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

A TOWN'S meeting was held at Liverpool last week for the purpose of hearing a report from the committee appointed a year or two ago as to the progress of a scheme for establishing a University College in Liverpool. The report showed that in spite of bad times a very gratifying readiness had been exhibited on the part of a number of the leading residents of Liverpool to contribute to the necessary funds, several of whom had promised sums of 10,000*l*. each for the endowment of different chairs. The Earl of Derby had also promised a similar sum, the result being that 80,000*l*. was already insured. The college is to be upon the broadest basis, being non-sectarian, and offering no disabilities of any kind to intending students. A resolution was moved thanking donors to the fund, pledging the meeting to the furtherance of the scheme, and recommending that the different classes and businesses of the town should form themselves into committees for the purpose of canvassing.

IN reply to a question by Sir J. Lawrence on Monday as to the embarrassed position of the United College of St. Andrews, Sir W. Harcourt stated that the Government would consider the report during the recess, with a view to making some proposal early next session.

A COMMITTEE is to be appointed to inquire as to the existing establishments which are available for intermediate and higher education in Wales.

THE Superior Council of Instruction in France has terminated its second session. The most notable feature has been the introduction of descriptive natural history in the eighth class, that is, in the first step of classical education.

SCIENTIFIC SERIALS

Annalen der Physik und Chemie, No. 6.—On electric expansion, by G. Quincke.—On the thermal and optical behaviour of gases under the influence of electric discharges, by E. Wiedemann.—On the electro-magnetic rotation of the plane of polarisation of light in gases, by A. Kundt and W. C. Röntgen.—On the theory of inconstant galvanic elements, by F. Exner.—On the specific heat of water, by A. Wüllner.—On the specific heat of mixtures of acetic acid and water, by M. A. von Reiss.—On a changed form of my proof of Maxwell's law of distribution of energy, by O. E. Meyer.—Researches on heat-conduction in liquids (continued), by H. F. Weber.—On anomalous dispersion in glowing sodium-vapour, by A. Kundt.—On a simple method of galvanic calibration of a wire, by V. Strouhal and C. Barus.—Explosive actions by ice, by Ed. Hagenbach.—On the funnel-valve in evacuated tubes, by W. Holtz.

No. 7.—Experiments on stationary vibrations of water, by G. Kirchhoff and G. Hansemann.—On the nature of galvanic polarisation, by W. Beetz.—Key for electric circuits, by the same.—On electric expansion (continued), by G. Quincke.—Experiments for determination of an upper limit for the kinetic energy of the electric current, by H. R. Hertz.—On fluorescence, by E. Lommel.—Researches on heat-conduction in liquids, by H. F. Weber.—On the transverse vibrations of a bar of variable cross-section, by G. Kirchhoff.